falls off much more rapidly than according to the formula. These observations are significant in view of the observation by the present Lord Rayleigh that the scattering power of saturated carbon di-oxide vapour at 21° C. is only 102 times that of the gas at atmospheric pressure, whereas according to the Einstein-Smoluchowski formula, it should have been 855 times as great.

The failure of the formula indicated above is especially surprising in view of its successes in other directions, namely, in the case of gases obeying Boyle's law, in the case of liquids under ordinary conditions, and, with certain restrictions, even in the case of solids. In attempting to find an explanation of the failure, at first sight one naturally seeks to find some flaw in Einstein's theory, or in the application of it, but the very successes of the formula in other cases would tend to discourage such an attempt. The formula was deduced by Einstein by applying Boltzmann's principle of entropy-probability in order to find the magnitude of the fluctuations of density of the fluid arising from thermal agitation and deducing the light-scattering due to these fluctuations by application of Maxwell's electromagnetic equations. It is clear that density fluctuations due to thermal agitation must occur; that their magnitude is proportional to the square root of the compressibility of the medium as contemplated in the theory may be confirmed independently by identifying the thermal energy of the molecules with the energy of sound-waves of all possible wave-lengths in an enclosed volume of the fluid and equating the energies. Further, the idea that the non-uniformity of the density of the medium is the factor determining light-scattering, at least according to the wave-theory, is confirmed by the very complete analysis of the problem given by the late Lord Rayleigh in one of his final papers (*Phil. Mag.*, Dec. 1918, p. 449). How, then, are we to escape the difficulty?

A very luminous suggestion made by Jeans in his "Dynamical Theory of Gases" (page 203) is here of great help. Jeans distinguishes between two kinds of clustering in fluid media, *mass-clustering* and *molecular-clustering*, and points out that they tend to become identical at the critical temperature. Einstein's theory is based on the idea that the fluctuations of density and the resulting scattering of light are both due to *mass-clustering*. If, however, we assume that it is *molecular-clustering* that is of importance and results in an increased scattering of light, it is easy to see that in the case of molecules such as carbon di-oxide, which are ordinarily non-associated, the clustering of molecules would only be appreciable near the critical temperature, and that at lower temperatures the clusters would rapidly break up and resolve themselves into single molecules. A double molecule would scatter four times as strongly as a single molecule, a triple molecule nine times as strongly, and so on, and if we assume that the energy-effects of separate molecules or groups are additive, and calculate the number of associated molecules from thermodynamic principles, it is easy to give the theory quantitative expression and explain the increased scattering near the critical point, and the rapid fall at lower temperatures.

But the fundamental difficulty remains, why the *mass-clustering* considered by Einstein does not, as it should, according to the classical wave-theory of light, give rise to an increased scattering of light?

To the present writer, at any rate, it appears that this contradiction of the electromagnetic theory by experience may have to be classed with its other known failures in the theory of photo-electricity and other modern fields of inquiry. We may, in fact, have to adopt the quantum theory of the structure of light as propagated in space (and not only when it is absorbed or emitted) in order to explain the facts of molecular diffraction. Fuller experimental data which are now being obtained in the writer's laboratory may pave the way towards the clearing up of this fundamental question.

C. V. Raman.

210 Bowbazar Street, Calcutta, March 2, 1922.

The Radiant Spectrum.

Prof. Raman in his reply of February 9 to my criticism of his first letter of September 1, does not refer to the fundamental difference of opinion between us. For it was the statement "the phenomenon is due to diffraction by the corneal corpuscles," to which I took exception, because I could not find in his letter, or in Brewster's paper, any evidence on which such a conclusion could be based.

With regard to the corneal corpuscles, Schafer writes in his "Essentials of Histology" (p. 363, edition 6), "Between the laminæ (of the cornea) lie flattened connective tissue corpuscles, which are branched and united by their processes into a continuous network; there is, of course, a corresponding network of cell spaces." Since, then, the corneal corpuscles lie within the substance of the cornea, their optical effect will depend on their opacity to light, or on the difference between their refractive index and that of their surroundings. Now if there was opacity, or a difference in refractive index, they should be visible under the microscope. But such is apparently not the case. Staining with hæmatoxylem or some other suitable reagent, is necessary in order that they may be visible, and therefore their opacity, or difference in R.I., must be slight. We conclude, therefore, that they will cause but slight diffraction in a ray of light passing through the cornea. In shape the cells themselves are *highly* irregular, and they average in man 20-30 μ in diameter. Their nuclei in man are roughly oval in shape, about 16 μ in diameter. In order that these structures should produce the type of diffraction pattern described by Prof. Raman, there should be two sets of them, nearly circular in outline, with diameters of 13 μ and 7 μ respectively. But a further point arises: Prof. Raman describes slight relative movements on the part of the diffraction pattern, which he compares with those which occur when a film of milk on glass is held in front of the eye. This movement, he states, ceases if the eyelids and eyeball be kept motionless for a short time. Could the corneal corpuscles undergo this movement lying as they do in lacunæ in the substance of the cornea? And even if they could, why should their motion cease when the lids and eyes are kept still?

Not only has no evidence been advanced by Prof. Raman in support of his statement that the corneal corpuscles are responsible for the diffraction phenomena, but also the shape, size, situation, and optical properties of these structures would appear to be antagonistic to the view.

With regard to the scattering of light by a prism, the following experiment will be found to demonstrate the effect. On the bed of a spectrometer are placed, base to apex, two glass prisms of equal dispersion, with optically good and clean surfaces (see Fig. 1).

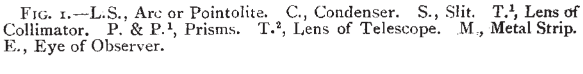

Fig. 1.—L.S., Arc or Pointolite. C., Condenser. S., Slit. T.^{1}, Lens of Collimator. P. & P.^{1}, Prisms. T.^{2}, Lens of Telescope. M., Metal Strip. E., Eye of Observer.

The telescope eye-piece having been removed, a

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narrow vertical strip of black sheet-metal or cardboard is fixed across the mouth of the telescope with plasticine, and the telescope is adjusted until the bright image of the slit is completely obstructed. On looking past the strip towards the prisms the observer will see parts of the latter brilliantly lit. By slightly shifting each prism in turn and watching for movement in the bright specks, those due to each prism are readily identified. H. Hartridge.

King's College, Cambridge.

Land Snails of the Madeira Islands.

In 1892 (*Journal of Conchology*, vol. vii. No. 1) the Rev. R. Boog Watson published a very interesting discussion of the Madeira snails, in which he raised many questions concerning their origin and history, which he did not attempt to answer. To-day, we are still groping for light, but I believe we may reach a number of conclusions which are not likely to prove erroneous.

(1.) It is not true that the numerous endemic snails of the islands have come "without trace of descent." They are quite clearly of Palæarctic origin, and their ancestors may be looked for in the rocks of Europe. Among the European Tertiary fossils, *Plebecula ramondi* Brong., from the Miocene of Germany, resembles the Madeira and Porto Santo Pleistocene fossil *P. bowditchiana*; *Pseudocampylaea insignis* Klein, from the Miocene of Würtemberg, resembles the great *P. lowei* of Porto Santo, but is not so large; two species of Craspedopoma, apparently allied to Madeira forms, are found in the Eocene of France. These fossils may be seen in the British Museum. In the absence of anatomical evidence, the relationships of these fossils must remain somewhat uncertain, but it is at least probable that their apparent affinity with the Madeira snails is not wholly deceptive.

(2.) Considering the diversity and strong peculiarities of most of the Madeira species, it is unlikely that their ancestors arrived later than the beginning of the Tertiary, and it is not improbable that at least part of the immigration dates from the Upper Cretaceous. This postulates a greater age for the islands than our present geological information can confirm. According to this view, the European fossils are presumably not the actual ancestors of the Madeira fauna, but derivatives from the same general stock.

(3.) A comparison between the snails of Madeira and the adjacent island of Porto Santo, which is easily visible from the coast of the larger island, shows that we have two very distinct faunæ, with very few species in common. More than this, various genera or subgenera are restricted to one of these islands, or specially characteristic of one. It is, I think, quite certain that during the whole long history of the snail fauna, Madeira and Porto Santo were never united. On the other hand, everything indicates that the three Desertas were during Tertiary time joined together and continuous, or nearly continuous, with Madeira. The following facts are illustrative:

Pseudocampylæa. Two species, Porto Santo only.

Cryptaxis. Three species, Madeira and Desertas (Leptaxis differs anatomically, as I shall show elsewhere).

Katostoma. Several forms, Porto Santo and adjacent islets.

Lampadia. One species, Porto Santo. (The Madeiran *membranacea* does not belong here.)

Idiomela. One species, Porto Santo.

Hispidella. One species, Madeira.

Lemniscia. One species, Porto Santo. (I am satisfied that the Madeira *calva* and *galeata* are not related.)

Actinella. Five species, Madeira and Desertas.

Hystricella. Seven species, Porto Santo and adjacent islets.

Geomitra. Seven species, Madeira and Desertas.

This list could be extended, but it is, I think, sufficient to indicate, not only that Madeira and Porto Santo were not united, but that they were never united with the mainland. The diversity of the genera and subgenera on the two islands might be expected as the result of accidental colonisation at very rare intervals, but could scarcely result from the breaking up of an originally homogeneous fauna. If we employ the aggregate genera Leptaxis and Geomitra (properly Ochthephila Beck, which is not preoccupied) without subdivision, the actual facts are obscured.

(4.) Nevertheless, we have to account for the occurrence of a certain number of identical species in the two islands, and the fact that some of the groups, such as Discula, are well represented on both islands, with closely allied species. The identical species were not introduced by man, as they occur fossil in the Pleistocene of Caniçal. As a general rule, when a group has representatives on both islands, it appears to be primarily or primitively a Porto Santo group. An apparent exception is Callina, with four Madeira species and one (*rotula*) in Porto Santo. The species *rotula* is very peculiar, and probably should not be associated with the others. How these various snails or their ancestors crossed the 23 miles from one island to the other is unknown. Some may have been carried by birds, possibly some may have come on floating pumice or other floating objects.[1] The reason for the apparent tendency of Porto Santo types to reach Madeira, rather than the reverse, may be found in the fact that the arid eastern end of Madeira is well suited to Porto Santo species, while Porto Santo is unsuited to the species from the moister uplands or coasts of the greater part of Madeira.

(5) The occurrence of well-defined species and subspecies on the islets around Porto Santo—some of them no larger than a large building—proves that no important oscillations of level have taken place in recent geological time. Very moderate alterations of level would submerge the islets, or unite them with the main island. The existence of these distinct forms on islets close to the main island also proves that the means of crossing the sea, whatever they are, operate at extremely infrequent intervals.

(6.) With regard to the species of the Madeiras which are actually identical with those of Europe, it must be said that the presumption is in nearly every case that they were introduced by man. It is possible, however, that some of the smaller ones were brought by "natural" means in geologically recent times, and highly probable that Balea was so brought to Porto Santo, on the feet of birds. Records of the occurrence of European species in the Pleistocene deposits of Madeira and Porto Santo all break down on critical examination.

That the islands are really "oceanic" is indicated by the total absence of indigenous mammals (except bats) and amphibians, and the general character of the invertebrate fauna and of the flora. The multitude of snails has seemed to suggest a former land connection, but I now believe that the snails themselves negative this view. T. D. A. Cockerell.

University of Colorado, Boulder, March 2.

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[1] Experiments should be made to determine whether it is even possible for snails protected by epiphragm or operculum, to pass alive through the alimentary canal of birds. Compare Wallis Kew, "The Dispersal of Shells," p. 45.

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